Technical Paper 266

A MODEL FOR USING QUALITATIVE VARIABLES AS COVARIATES IN THE ANALYSIS OF COVARIANCE

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INDIVIDUAL TRAINING & PERFORMANCE EVALUATION TECHNICAL AREA

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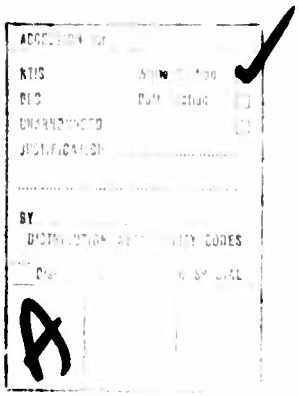

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The powers of fixed effects randomized block (RB) and analysis of covariance (CANCOVA) using qualitative concomitant variables were analytically and empirically compared. Analytical comparisons were made of the powers of RB and CANCOVA in which the number of observations (n_i) within each of the I categories of the concomitant variable was a constant. Empirical comparisons were made of the power of CANCOVA in which n_i was a

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20. random variable (RCANCOVA) with RB in which n_{ij} was a constant. A Monte Carlo program simulated fixed effects analyses with two levels of treatment, one criterion variable, and a qualitative concomitant variable with I categories. Three "design types" in which I was equal to 2, 3, and 4 were studied. The parameters varied for each design type were: (1) total sample size (n..) (I=2, n..=20, 80; I=3, n..=36, 144; I=4, n..=56, 224); (2) ratio of number of row observations (I=2, 1:1, 4:1; I=3, 1:1:1, 4:1:1; I=4, 1:1:1:1, 4:1:1:1); (3) eta (0.0, 0.3, 0.9); and (4) magnitude of treatment effect (0.0, 0.2, 0.5).

Analytically, the RB and CANCOVA provided the same information in terms of component sums of squares. However, the power relationship was shown to be a function of sample size, design type, and amount of heterogeneity (interaction) present. Empirically no interpretable differences were found, either in magnitude and direction, between the power of the RB and RCANCOVA for any of the design type and parameter combinations studied.

Technical Paper 266

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FOREWORD

The Army Research Institute for the Behavioral and Social Sciences (ARI) has developed a wide range of statistical models to test hypotheses generated in relation to an equally wide range of measurement and evaluation situations. The powerful Randomized Block (RB) design has traditionally been a preferred model for much psychological research. The RB design has the stringent requirement, however, that the sample population be strictly defined and stratified beforehand, a requirement more appropriate in a controlled laboratory environment than in many Army field situations. This Technical Paper describes the development of an alternative statistical design which provides the advantages of the classic RB method without its operational disadvantages, and which will be useful not only in the Individual Training and Performance Evaluation Technical Area in which it was developed but in other areas of behavioral science research.

The entire research is responsive to requirements of RDTE Project 2Q762717A745, Selection and Individual Training Research, FY 1975 Work Program, and to special requirements of the Deputy Chief of Staff for Personnel.

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A MODEL FOR USING QUALITATIVE VARIABLES AS COVARIATES IN THE ANALYSIS OF COVARIANCE

BRIEF

Requirement:

To develop, as an alternative to the traditional Randomized Block (RB) two-way analysis of variance design, an equally efficient statistical model that will eliminate the RB's requirement for a priori stratification and sampling and, at the same time, retain the RB's ability to handle categorical concomitant variables. That is, to develop a statistical design with the advantages of the classic RB method without its operational disadvantages.

Procedure:

The statistical model selected for comparison and test was a modified analysis of covariance (ANCOVA) design that does not require previously selected stratified samples and does incorporate the ability to handle categorical variables--the Categorical Analysis of Covariance (CANCOVA). The powers of fixed effects RB and CANCOVA using qualitative (categorical) concomitant variables were analytically and empirically compared. A Monte Carlo program simulated fixed effects analysis with two levels of treatment, one criterion variable, and a qualitative concomitant variable with three design types. The parameters which varied for each design type were sample size, ratio of numbers of row observations, eta, and magnitude of treatment effects.

Findings:

With relatively large samples, the RB and the CANCOVA designs yielded the same information in terms of component sums of squares. With small samples, the power relationship is a function of sample size, design type and amount of heterogeneity. Empirically, no practical difference was found between the power of RB and CANCOVA when the samples are large.

Utilization of Findings:

Where the population cannot be well defined or stratified because of necessary administrative and/or physical constraints, practical field experimentation can be undertaken with a precision comparable to the more expensive and laborious traditional RB design. The CANCOVA requires only that the subjects be sampled directly from the population and randomly assigned to the different experimental treatments.

A MODEL FOR USING QUALITATIVE VARIABLES AS COVARIATES IN THE ANALYSIS OF COVARIANCE

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A MODEL FOR USING QUALITATIVE VARIABLES AS COVARIATES IN THE ANALYSIS OF COVARIANCE

Experimental results are not only affected by treatments but by extraneous variation which often tends to mask the primary effects of experimental treatments. The effect of extraneous variation on experimental results is referred to as experimental error variance. In the behavioral sciences experimental error variance can be relatively large and influence the results of an experiment in such a manner that only large treatment effects can be detected, and even these may be subject to uncertainty. By the careful design of experiments, it is possible to control sources of extraneous variation, reducing experimental error variance and increasing the precision of the experiment (precision refers to the power or ability of a design to detect treatment effects).

One way of controlling experimental error variance is by capitalizing on relationships between the experimental or dependent variables and external concomitant variables. External concomitant variables are variables which are measured prior to experimentation and are not affected by the experimental treatment. For example, IQ could be used as an external concomitant variable to control for error variance due to difference in innate abilities. Two designs which employ external concomitant variables to control error variance are: (1) Randomized Block (RB), and (2) Analysis of Covariance (ANCOVA). The RB experimentally controls error variance by using the external concomitant variable to stratify the samples assigned to the treatment categories into homogeneous groups called blocks, while the ANCOVA statistically controls error variance by using the linear regression of the dependent experimental variable on the external concomitant variable.

Several researchers have compared the precision of RB and ANCOVA designs. Cochran[1] found precision was directly related to the correlation of the concomitant and dependent variables. For correlations of less than 0.3, the use of RB or ANCOVA to increase the precision of the experiment was inconsequential, but as the correlation increases towards unity, sizeable increases in precision are obtained. For large sample sizes, Cochran concluded that for experimental designs in which the relationship between the experimental and concomitant variables was linear, the precisions of the RB and ANCOVA are about the same. Cox[2] found that

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[1] Cochran, W. G. Analysis of covariance: Its nature and uses. Biometrika, 1957, 44, 261-281.

[2] Cox, D. R. The use of a concomitant variable in selecting an experimental design. Biometrika, 1957, 44, 150-158.

RB provided greater precision when the correlation between the variables was less than 0.6 and ANCOVA provided greater precision only when the correlation was greater than 0.8. Cox's conclusion applies to designs with relatively small sample sizes and blocking levels assigned on the basis of an underlying continuum, i.e., a rank ordering of the categories of the blocking variable. Feldt[3] studied designs in which each cell had at least two observations. (Cox's designs only had one observation per cell.) Feldt concluded that for correlations less than 0.4, RB resulted in approximately equal or greater precision than ANCOVA; for correlations greater than 0.6 ANCOVA was "superior."

The findings of the above studies are only applicable to designs in which the concomitant variable is continuous, i.e., a variable which can take on any value within a specified range. For example, weight is a continuous variable; it can take on any value within the possible range of values applicable to the object being weighed. The results of these studies do not apply to designs in which the concomitant variable is qualitative, i.e., a variable which is categorical, in that it categorizes or names; for example, different modes of instruction, racial differences, differences in geographic origin, or social class differences are all qualitative variables. When the concomitant variables are qualitative, it is traditional to use the RB technique. In the RB design the population is stratified into homogenous groups based on the categories of the qualitative concomitant variable. Once the population has been stratified, random samples of subjects are selected from each strata and assigned to the different experimental treatments. In a laboratory setting, with a well-defined population, a priori stratification of and random selection of subjects from the entire population is easily accomplished. However, in field experimentation, where the population is not well defined and a priori stratification of the entire population is difficult due to administrative and physical restrictions, employment of RB designs can be difficult or impossible. In many situations the possible gain in precision is far outweighed by the necessary effort and expense of employing the RB design.

The ANCOVA offers a possible alternative to the RB design. ANCOVA does not require a priori stratification and sampling; the subjects are sampled directly from the total population and randomly assigned to the different experimental treatments. However, the traditional ANCOVA model was developed under the assumption that the concomitant variables were random and continuous; as such, the traditional ANCOVA is not applicable in situations where the concomitant variables are qualitative and therefore categorical. The purpose of this research is to develop a categorical ANCOVA (CANCOVA), i.e., an ANCOVA which will allow the use of categorical variables, and to compare the precision of the CANCOVA with the traditional RB.

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[3] Feldt, L. S. A comparison of the precision of three experimental designs employing a concomitant variable Psychometrika, 1958, 23, 335-353.

Two of the most widely used techniques for increasing the precision of randomized experiments are: (1) stratification or blocking of experimental samples (RB), and (2) analysis of covariance (ANCOVA). This review is limited to studies in which the precisions of RB and ANCOVA were compared.

Cochran[4] showed that, for both RB and ANCOVA, the gain in precision over completely randomized designs was a function of the size of the correlation coefficient ρ_{xy} between the criterion variable Y and the concomitant variable X. If σ_y^2 is the experimental error variance when no adjustment is employed, then the adjustment by covariance reduces this variance to:

$$\sigma_y^2 (1 - \rho_{xy}^2) \left\{ 1 + \frac{1}{f_e - 2} \right\} \qquad (1)$$

where f_e is error degrees of freedom. The factor involving f_e is needed to take into account errors in the estimated regression coefficient for the bivariate sample. The adjustment by blocking reduces σ_y^2 to:

$$\sigma_y^2 (1 - \rho_{xy}^2). \qquad (2)$$

From equations 1 and 2 it is clear that for small values of ρ_{xy} ($\rho_{xy} < 0.3$), the gain in precision afforded by using either RB or ANCOVA is likely to be inconsequential, but as ρ_{xy} increases towards unity, sizeable increases in precision are obtained. Cochran concluded that for experimental designs in which the relationship between X and Y is linear, the precisions of the RB and ANCOVA are about the same (for large sample sizes).

Cox[5] used two measures of imprecision as a basis for comparing the relative efficiency of RB and ANCOVA. The first, true imprecision was based on the population value of the average error variance for the difference between two treatment means (adjusted for covariance when appropriate). The second, apparent imprecision was defined as the

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[4] Cochran, 1957, op. cit.

[5] Cox, 1957, op. cit.

product of the true imprecision and an adjustment factor[6] based on error degrees of freedom. The adjustment factor allows for a more meaningful comparison of the relative efficiency of two techniques which use the same number of observations but have different error degrees of freedom. Symbolically, these two indices are:

$$I_t = \bar{s}^2_{\bar{y}_i - \bar{y}_j} \Big/ [2\sigma_y^2 (1 - \rho_{xy}^2)/n]$$

$$I_a = I_t \left(\frac{f_e + 3}{f_e + 1} \right)$$

where $\bar{s}^2_{\bar{y}_i - \bar{y}_j}$ is the variance of the estimated difference between a pair of treatment means averaged over all pairs of treatment means, f_e is the error degrees of freedom, n is the number of observations per treatment group, σ_y^2 is the variance of Y within each treatment population, ρ_{xy} is the linear correlation coefficient between X and Y, and $[2\sigma_y^2 (1 - \rho_{xy})/n]$ is the minimum variance of the difference between treatment means averaged over all pairs of treatment means. For any pair of designs based on the same total sample size, comparison of the respective values of I_a will indicate which of the two designs is more efficient.

Cox evaluated I_t and I_a for RB and ANCOVA using several combinations of total sample size, ρ_{xy}, and numbers of levels of treatment (t). Cox concluded that RB provided greater precision when $\rho_{xy} < 0.6$, and ANCOVA provided greater precision only when $\rho_{xy} > 0.8$.

It should be noted that designs used by Cox were not fixed effects. Cox assumed that the blocking levels were selected randomly by ranking subjects on the blocking variable X, subdividing the ranked subjects into t groups, and assigning one subject per block at random to each of the t levels of treatment. Thus, the interaction of blocks and treatment provides an appropriate error term. On the other hand, designs used in behavioral research are typically fixed-effects models with more than one observation per cell.

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[6] Fisher, R. A. The design of experiments. London: Oliver and Boyd, 1949.

Feldt[7] extended Cox's study to designs in which the main and interaction effects were fixed and each cell had at least two observations. All designs considered by Feldt were completely randomized having t levels of treatment with an equal number of observations. The samples were assumed to have been drawn from t normally distributed populations with constant variance and a normally distributed concomitant X, linearly related to Y. The treatment population means and variances of the X variable and the within treatment correlation coefficients between X and Y were assumed to be equal across all treatment levels.

Feldt used the same indices of imprecision as Cox; however, they were modified to account for more than one observation per cell. Feldt concluded that for $\rho_{xy} < 0.4$ RB resulted in approximately equal or greater precision than ANCOVA; for $\rho_{xy} > 0.6$ ANCOVA was "superior." For relatively high values of ρ_{xy} and relatively small total sample size, the difference in precision in favor of ANCOVA was appreciable. This difference was attributed to the relatively small sample size not permitting the experimenter to employ a sufficiently large number of blocking levels to exploit fully the value of the concomitant variable in RB. Feldt noted that for $\rho_{xy} < 0.2$ and small sample sizes neither ANCOVA nor RB yielded appreciably greater precision than a completely randomized design.

In all articles reviewed, the concomitant variable, X, was assumed to be normally distributed. Further, none of the articles was addressed to the situation in which the concomitant variable was qualitative. Feldt's and Cox's indices of imprecision assume that the control variable had an underlying continuous distribution and that each category of the control variable had a definable variance. When X is qualitative the within-block variance of X is zero; therefore, these indices cannot be used to compare the precision of RB and ANCOVA when the concomitant variable is qualitative.

METHODOLOGY

The methodology discussion is divided into two sections, analytical and empirical. The analytical section involves a comparison of the power of RB and CANCOVA in which the number of observations (n_i) within each of the I categories of the concomitant variable is constant. The empirical section compares the power of CANCOVA in which n_i is a random variable (RCANCOVA) with RB in which n_i is a constant; sample size is held constant for both designs. This comparison simulates the practical

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[7] Feldt, 1958, op. cit.

circumstances surrounding the probable implementation of these designs. The introduction of n_i as a random variable complicates the mathematical models making it only feasible to compare the power of the RB and RCANCOVA designs by Monte Carlo methods.

Analytical

Development of the analytical models is based on the General Linear Model (GLM). Structural models for RB and CANCOVA were developed and appropriate parameter and design matrices were defined. Functional relationships were established between the RB and CANCOVA model components by comparing the parameter and design matrices of the respective models. Mathematical functions were developed for the RB and CANCOVA treatment effect F ratios. The power of the two models was compared by examining the relationship between the mathematical functions defining the respective treatment effect F probability distributions.

Empirical

The Generation of Random Normal Samples. The generation of random normal samples for RB and RCANCOVA was accomplished by using RANDN.[8] RANDN was called separately to generate sets of observations for each cell of a design. Each set of observations was generated from a population with a specified mean and standard deviation of 1. Cell means were computed from row mean values (row means are a function of the correlation coefficient, eta, for the specific design being run) and treatment effect differences. A FORTRAN program was used to compute the values of row means for each of the possible design combinations run; program documentation and a table of the row means used for each value of eta are presented in Appendix A.

Goodness-of-Fit Tests. The randomness and goodness of fit to normality of the samples generated by RANDN are dependent on the initiation number used in the generating process. Several starting numbers were tested for the fit of the numbers they generated to a hypothetical normal distribution by means of a chi-square goodness-of-fit test. Documentation for the chi-square program is presented in Appendix B. In addition to these tests, data were generated based on 3,000 samples, using each starting number, for the 12 possible design combinations in which eta and treatment effect were both 0. The goodness of fit of the empirical frequency of rejection of the null hypothesis of no treatment effect to the expected frequency of rejection under the central F distribution was determined for six nominal alpha levels .01, .05, .10, .25, .50, .75 using a 5-degree-of-freedom chi-square goodness-of-fit test.

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[8] University of Maryland UNIVAC 1108 EXEC 8 Math-Pack users' guide. College Park, Md.: Computer Science Center, University of Maryland, 1970.

Empirical Power Comparisons. Only designs in which I was equal to 2, 3, and 4 were studied. Several combinations of parameters were used with each of these three design types ("design type" refers to the number of categories in the concomitant variable). The parameters which were varied for each design type were: (1) sample size, (2) ratio of number of row observations, (3) eta, and (4) magnitude of treatment effect. Table 1 lists the values assigned to each parameter for each of the three design types.

Table 1

PARAMETERS FOR EACH OF THE THREE DESIGN TYPES

Parameter	Design Type		
	Two-Category Variable Design	Three-Category Variable Design	Four-Category Variable Design
Sample Size	20	36	56
	80	144	224
Marginal Ratio			
equal n	1:1	1:1:1	1:1:1:1
unequal n	4:1	4:1:1	4:1:1:1
Eta	0.0	0.0	0.0
	0.3	0.3	0.3
	0.9	0.9	0.9
Magnitude of Treatment Effect	0.0	0.0	0.0
	0.2	0.2	0.2
	0.5	0.5	0.5

Two sets of 3,000 sample data generations were run for each of the 48 design combinations in which the values for eta and the treatment effect were non-zero. One set of data generations was analyzed by the RB subroutine and the other by the RCANCOVA subroutine of the Monte Carlo program (Appendix C). For each run, empirical F ratios for treatment, interaction (RB), and heterogeneity of regression (RCANCOVA) were computed along with their respective probabilities under the null hypotheses of no treatment effect, no interaction effect, and homogeneity of regression. Each of the computed probabilities for the treatment effect F ratios was compared to the 6 nominal alpha levels, and rejection rates were tabulated for each alpha level. For each sample generated, the computed probability for interaction and heterogeneity of regression F ratios was compared to the .01 and .05 alpha levels; those samples with interaction or heterogeneity of

regression significant at the .01 and .05 levels were tabulated as subgroups. The empirical rejection rate for the treatment effect F ratios in each subgroup was tabulated in the same manner as described for the total set of samples.

The empirical power of a given design combination for a specified alpha level is equal to the proportion of times the null hypothesis of no treatment effect is rejected. The empirical powers of RB and RCANCOVA were compared at each of the six nominal alpha levels. The statistical significance of the comparisons was determined by using either of two statistics; the first is a z statistic defined by Walker and Lev:[9]

$$z = \frac{P_1 - P_2}{\sqrt{Npq/N_1 N_2}}$$

where N_1 is the number of cases in population 1 for which the observed proportion of rejection is p_1, N_2 is the number of cases in population 2 for which the observed proportion of rejections is p_2, $p = p_1 + p_2$, $q = 1 - p$, and $N = N_1 + N_2$; the second is a z' statistic defined by Haight:[10]

$$z' = \frac{y - x}{\sqrt{y + x}}$$

where y is the frequency of rejections in population 1 and x is the frequency of rejections in population 2. The z statistic is approximately a random variable with a normal distribution and the z' statistic is approximately a random variable with a Poisson distribution. Since the binomial test becomes skewed and the normal approximation is inaccurate for proportions close to 0 or 1, the z statistic was used for comparisons in which the population proportions were close to .5, and the z' statistic was used for comparisons in which the population proportions were close to 1 or 0.

RESULTS

Analytical

The following conventions were used in developing the analytical arguments:

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[9] Walker, H. M., and Lev, J. Statistical inference. New York: Holt, Rinehart and Winston, 1953.

[10] Haight, F. A. Handbook of the Poisson distribution. New York: Wiley, 1967.

1. Superscripts designate the model or design type: f = full model, r = reduced model, c = CANCOVA, and b = RB.

2. Subscripts index a variable within a specific design: i = category of control variable, j = level of the treatment dimension, and k = individual within an ij combination.

3. Matrix notation is condensed by writing a column vector as its transpose row vector; for matrices involving repetition of elements, the following column vector notation is used: $\underline{A}n_j$ is a column vector of n_j A's; e.g., $\underline{1}_9$ is a column vector of 9 1's.

The RB model is a two-way analysis of variance design in which the levels of the blocking variable correspond to the categories of the qualitative control variable. In the CANCOVA model the categories of the qualitative control variable are translated into sets of dichotomous covariables.[11] If X_m (m = 1, 2, ..., I-1) is the m^{th} covariable score in the i^{th} (i = 1, 2, ..., I) category of the qualitative control variable, then the value of X_m for m = i is d, and the value of X_m for m ≠ i is g; the values of d ≠ g are arbitrary, e.g., d = 1 and g = 0, or d = -1 and g = 1, etc. Table 2 schematically represents the covariable allocation or blocking strategy for a design in which the qualitative control variable has I categories.

ANOVA designs can be represented as special forms of the general linear model (GLM).[12] The matrix form for a sample of n scores based on a model with p < n parameters is

$$\underline{Y} = \underline{X}\,\underline{\theta} + \underline{e} ,$$

where $\underline{Y}$ is an (nx1) vector of random observations, $\underline{X}$ is an (nxp) design matrix of known quantities, $\underline{\theta}$ is a (px1) vector of unknown parameters, and $\underline{e}$ is an (nx1) vector of unobserved random errors, normally distributed with $E(\underline{e}) = 0$ and $E(\underline{e}\,\underline{e}') = I\sigma^2$, where I is the (nxn) identity matrix and σ^2 is the variance. $\underline{Y}$ and $\underline{X}$ represent observable data, whereas $\underline{\theta}$ and $\underline{e}$ are unknown. The least squares estimator of $\underline{\theta}$ is defined such that

$$\underline{e}'\underline{e} = \text{minimum.}$$

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[11] Suits, D. B. Use of dummy variables in regression equations. Journal of the American Statistical Association, December 1962, 548-551.

[12] Dayton, C. M. An introduction to the general linear model. University of Maryland, Department of Measurement and Statistics Monograph, College Park, Maryland, 1969.

Table 2

SCHEMATIC OF RB (Ix2) OR CANCOVA (Ix2 WITH I-1 COVARIABLES)

Dichotomous Dummy Covariables (X_m)					Treatment		
X_1	X_2	...	X_{I-1}	Blocks	1	2	
1	0	...	0		Y_{111}	Y_{121}	
.	.		.		.	.	
.	.	...	.	1	.	.	$\bar{Y}_{1\cdot\cdot}$
.	.		.		.	.	
1	0	...	0		Y_{11n_1}	Y_{12n_1}	
	.			.	.	.	.
	.			.	.	.	.
	.			.	.	.	.
0	0	...	1		$Y_{I-1,11}$	$Y_{I-1,\ 21}$	
.	.		.		.	.	
.	.	...	.	I-1	.	.	$\bar{Y}_{I-1\cdot\cdot}$
.	.		.		.	.	
0	0	...	1		$Y_{I-1,1n_{I-1}}$	$Y_{I-1,2n_{I-1}}$	
0	0	...	0		Y_{I11}	Y_{I21}	
.	.		.		.	.	
.	.	...	.	I	.	.	$\bar{Y}_{I\cdot\cdot}$
.	.		.		.	.	
0	0	...	0		Y_{I1n_I}	Y_{I2n_I}	
					$\bar{Y}_{\cdot 1\cdot}$	$\bar{Y}_{\cdot 2\cdot}$	

The value of $\hat{\underline{\theta}}$ which minimizes $\underline{e}'\underline{e}$ is found by the solution of

$$\frac{\partial (\underline{e}'\underline{e})}{\partial \theta_t} = 0$$

where t = 1, 2, ..., p. The solution results in a system of normal equations:

$$\underline{X}\,\underline{X}'\hat{\underline{\theta}} = \underline{X}'\underline{Y}$$

If $\underline{X}$ is nonsingular, then $(\underline{X}'\underline{X})^{-1}$ exists and there is a unique solution

$$\hat{\underline{\theta}} = (\underline{X}'\underline{X})^{-1}\underline{X}'\underline{Y}$$

The model for a score in the RB analysis is

$$Y_{ijk} = \mu^{(fb)} + \alpha_j^{(fb)} + \beta_i^{(fb)} + \gamma_{ij}^{(fb)} + e_{ijk}^{(fb)}$$

where the superscript (fb) denotes full model for RB, $\mu^{(fb)}$ is an additive constant or grand mean, $\alpha_j^{(fb)}$ (j = 1, 2, ..., J) is the effect of being in the j^{th} level of the treatment dimension, $\beta_i^{(fb)}$ (i = 1, 2, ..., I) is the effect of being in the i^{th} blocking level, $\gamma_{ij}^{(fb)}$ is the interaction effect of being in the ij^{th} cell of the design, and $e_{ijk}^{(fb)}$ is the error effect associated with the k^{th} observation in the ij^{th} cell of the design. If the parameter and design matrices are defined using this model, the design matrix will be singular with column rank JI. Since the smaller order of this design matrix is (J+1)(I+1), in order to remove the singularity a total of (J+I+1) restrictions is needed. These restrictions can be generated from three classes of linear restrictions:

$$\sum_{j=1}^{J} \alpha_j = 0 \qquad (3)$$

$$\sum_{i=1}^{I} \beta_i = 0 \qquad (4)$$

$$\sum_{i=1}^{I} \sum_{j=1}^{J} \gamma_{ij} = 0 \qquad (5)$$

Under these restrictions, appropriate parameter and design matrices are:[13]

$$\underset{\sim}{\theta}^{(fb)\prime} = \left[\mu^{(fb)}, \alpha_1^{(fb)}, \beta_1^{(fb)}, \beta_2^{(fb)}, \ldots, \beta_{I-1}^{(fb)}, \gamma_{11}^{(fb)}, \gamma_{12}^{(fb)}, \ldots, \gamma_{1,I-1}^{(fb)}\right]$$

$$\underset{\sim}{X}^{(fb)} = \begin{bmatrix}
1_{n_{11}} & 1_{n_{11}} & 1_{n_{11}} & 0_{n_{11}} & \cdots & 0_{n_{11}} & 1_{n_{11}} & 0_{n_{11}} & \cdots & 0_{n_{11}} \\
1_{n_{21}} & 1_{n_{21}} & 0_{n_{21}} & 1_{n_{21}} & \cdots & 0_{n_{21}} & 0_{n_{21}} & 1_{n_{21}} & \cdots & 0_{n_{21}} \\
\vdots & \vdots & \vdots & \vdots & \ddots & \vdots & \vdots & \vdots & \ddots & \vdots \\
1_{n_{I-1,1}} & 1_{n_{I-1,1}} & 0_{n_{I-1,1}} & 0_{n_{I-1,1}} & \cdots & 1_{n_{I-1,1}} & 0_{n_{I-1,1}} & 0_{n_{I-1,1}} & \cdots & 1_{n_{I-1,1}} \\
1_{n_{I1}} & 1_{n_{I1}} & 0_{n_{I1}} & 0_{n_{I1}} & \cdots & 0_{n_{I1}} & 0_{n_{I1}} & 0_{n_{I1}} & \cdots & 0_{n_{I1}} \\
1_{n_{12}} & -1_{n_{12}} & 1_{n_{12}} & 0_{n_{12}} & \cdots & 0_{n_{12}} & -1_{n_{12}} & 0_{n_{12}} & \cdots & 0_{n_{12}} \\
1_{n_{22}} & -1_{n_{22}} & 0_{n_{22}} & 1_{n_{22}} & \cdots & 0_{n_{22}} & 0_{n_{22}} & -1_{n_{22}} & \cdots & 0_{n_{22}} \\
\vdots & \vdots & \vdots & \vdots & \ddots & \vdots & \vdots & \vdots & \ddots & \vdots \\
1_{n_{I-1,2}} & -1_{n_{I-1,2}} & 0_{n_{I-1,2}} & 0_{n_{I-1,2}} & \cdots & 1_{n_{I-1,2}} & 0_{n_{I-1,2}} & 0_{n_{I-1,2}} & \cdots & -1_{n_{I-1,2}} \\
1_{n_{I2}} & -1_{n_{I2}} & 0_{n_{I2}} & 0_{n_{I2}} & \cdots & 0_{n_{I2}} & 0_{n_{I2}} & 0_{n_{I2}} & \cdots & 0_{n_{I2}}
\end{bmatrix}$$

[13] To conserve space the design and parameter matrices illustrated throughout the rest of this paper are restricted to designs in which the treatment dimension has two levels; this restriction does not preclude generalization of the results to designs with more than two levels of the treatment dimension.

The model for the same score under the CANCOVA analysis using separate within-cell regression is

$$Y_{jk} = \mu^{(hc)} + \alpha_j^{(hc)} + \sum_{i=1}^{I-1} b_{ij} X_{ijk} + e_{jk}^{(hc)}$$

where (hc) is a superscript denoting a CANCOVA design using separate within-cell regression, $\mu^{(hc)}$ is an additive constant, $\alpha_j^{(hc)}$ is the effect of being in the j^{th} level of the treatment dimension, b_{ij} is the regression coefficient due to the regression of the i^{th} covariable within the j^{th} treatment level on the Y scores within the j^{th} treatment level, X_{ijk} is the value of the i^{th} covariable score for the k^{th} observation within the j^{th} treatment level, and $e_{jk}^{(hc)}$ is the random error for the k^{th} observation within the j^{th} level of treatment. The design matrix using this model will be singular with column rank JI. Since the smaller order of the design matrix is JI+1 to remove the singularity 1 restriction is needed:

$$\sum_{j=1}^{J} \alpha_j = 0$$

Under this restriction, appropriate parameter and design matrices are:

$$\underline{\theta}^{(hc)\prime} = \left[\mu^{(hc)}, \alpha_1^{(hc)}, b_{11}, b_{21}, \ldots, b_{I-1}, b_{12}, b_{22}, \ldots, b_{I-1,2}\right]$$

$$\underline{X}^{(hc)} = \begin{bmatrix}
1_{n_{11}} & 1_{n_{11}} & 1_{n_{11}} & 0_{n_{11}} & \cdots & 0_{n_{11}} & \ddots & \vdots & \iddots \\
1_{n_{21}} & 1_{n_{21}} & 0_{n_{21}} & 1_{n_{21}} & \cdots & 0_{n_{21}} & & & \\
\vdots & \vdots & \vdots & \vdots & \ddots & \vdots & \cdots & 0 & \cdots \\
1_{n_{I-1,1}} & 1_{n_{I-1,1}} & 0_{n_{I-1,1}} & 0_{n_{I-1,1}} & \cdots & 1_{n_{I-1,1}} & & & \\
1_{n_{I1}} & 1_{n_{I1}} & 0_{n_{I1}} & 0_{n_{I1}} & \cdots & 0_{n_{I1}} & \iddots & \vdots & \ddots \\
1_{n_{12}} & -1_{n_{12}} & \ddots & \vdots & \iddots & 1_{n_{12}} & 0_{n_{12}} & \cdots & 0_{n_{12}} \\
1_{n_{22}} & -1_{n_{22}} & & & & 0_{n_{22}} & 1_{n_{22}} & \cdots & 0_{n_{22}} \\
\vdots & \vdots & \cdots & 0 & \cdots & \vdots & \vdots & \ddots & \vdots \\
1_{n_{I-1,2}} & -1_{n_{I-1,2}} & & & & 0_{n_{I-1,2}} & 0_{n_{I-1,2}} & \cdots & 1_{n_{I-1,2}} \\
1_{n_{I2}} & -1_{n_{I2}} & \iddots & \vdots & \ddots & 0_{n_{I2}} & 0_{n_{I2}} & \cdots & 0_{n_{I2}}
\end{bmatrix}$$

Both $\underline{X}^{(fb)}$ and $\underline{X}^{(hc)}$ are nonsingular matrices with column rank JI. Since both are based on full-rank models, they account for the same amount of the total variance and $SS_e^{(fb)} = SS_e^{(hc)}$.[14, 15]

An alternative to the (hc) CANCOVA is a model in which the pooled within-cell regression coefficient is used instead of separate within-cell regression coefficients. The model for a score is

$$Y_{jk} = \mu^{(c)} + \alpha_j^{(c)} + \sum_{i=1}^{I-1} b_i X_{ijk} + e_{jk}$$

where (c) denotes the CANCOVA model in which the pooled within-cell regression coefficient is used, $\mu^{(c)}$ is an additive constant, $\alpha_j^{(c)}$ is the effect of being in the j^{th} level of the treatment dimension, b_i is the pooled within-cell regression coefficient due to the regression of the i^{th} covariable on the Y scores, X_{ijk} is the value of the i^{th} covariable score for the k^{th} individual in the j^{th} level of the treatment, and e_{jk} is the random error for the k^{th} observation within the j^{th} treatment level. The design matrix for this model will be singular with column rank J+I+1. Since the smaller order of this design matrix is J+I, in order to remove the singularity, 1 restriction is needed:

$$\sum_{j=1}^{J} \alpha_j = 0$$

Under this restriction the appropriate parameter and design matrices are:

$$\underline{\theta}^{(c)\prime} = \left[\mu^{(c)}, \alpha_1^{(c)}, b_1, b_2, \ldots, b_{I-1}\right]$$

---

[14] Scheffé, H. The analysis of variance. New York: John Wiley & Sons, 1959.

[15] Graybill, F. A. An introduction to linear statistical models. New York: McGraw-Hill, 1961. Pp. 106-145.

$$
\underline{X}^{(c)} = \begin{bmatrix}
\underline{1}_{n_{11}} & \underline{1}_{n_{11}} & \underline{1}_{n_{11}} & \underline{0}_{n_{11}} & \cdots & \underline{0}_{n_{11}} \\
\underline{1}_{n_{21}} & \underline{1}_{n_{21}} & \underline{0}_{n_{21}} & \underline{1}_{n_{21}} & \cdots & \underline{0}_{n_{21}} \\
\vdots & \vdots & \vdots & \vdots & \ddots & \vdots \\
\underline{1}_{n_{I-1,1}} & \underline{1}_{n_{I-1,1}} & \underline{0}_{n_{I-1,1}} & \underline{0}_{n_{I-1,1}} & \cdots & \underline{1}_{n_{I-1,1}} \\
\underline{1}_{n_{11}} & \underline{1}_{n_{11}} & \underline{0}_{n_{11}} & \underline{0}_{n_{11}} & \cdots & \underline{0}_{n_{11}} \\
\underline{1}_{n_{12}} & -\underline{1}_{n_{12}} & \underline{1}_{n_{12}} & \underline{0}_{n_{12}} & \cdots & \underline{0}_{n_{12}} \\
\underline{1}_{n_{22}} & -\underline{1}_{n_{22}} & \underline{0}_{n_{22}} & \underline{1}_{n_{22}} & \cdots & \underline{0}_{n_{22}} \\
\vdots & \vdots & \vdots & \vdots & \ddots & \vdots \\
\underline{1}_{n_{I-1,2}} & -\underline{1}_{n_{I-1,2}} & \underline{0}_{n_{I-1,2}} & \underline{0}_{n_{I-1,2}} & \cdots & \underline{1}_{n_{I-1,2}} \\
\underline{1}_{n_{12}} & -\underline{1}_{n_{12}} & \underline{0}_{n_{12}} & \underline{0}_{n_{12}} & \cdots & \underline{0}_{n_{12}}
\end{bmatrix}
$$

If the data are such that for the i^{th} covariable $b_{ij} = b_{i,j+1}$ for $j = 1, \ldots, J-1$, then the (c) and the (hc) CANCOVA models are identical and account for the same amount of total variance; however, if for the i^{th} covariable $b_{ij} \neq b_{i,j+1}$ for some value of j, then the column rank of $\underline{X}^{(hc)}$ will be greater than the column rank of $\underline{X}^{(c)}$ and the difference in the amount of total variance accounted for by the two models will be reflected in the difference between their respective error sum of squares.[16] In general, the difference between the error sum of squares for the two models is

$$SS_e^{(c)} - SS_e^{(hc)} = SS_h \qquad (6)$$

where SS_h is the sum of squares due to heterogeneity.

A similar approach can be used with the RB model to define the sum of squares for interaction ($SS_\gamma^{(rb)}$). If $\gamma_{ij} = 0$ for all i, j, then the appropriate parameter and design matrices for this reduced model are:

$$\underline{\theta}^{(rb)} = \left[\mu^{(rb)}, \alpha_1^{(rb)}, \beta_1^{(rb)}, \beta_2^{(rb)}, \ldots, \beta_{I-1}^{(rb)}\right]$$

[16] Graybill, 1961, op. cit.

$$
\underline{X}^{(rb)} = \begin{matrix}
\underline{1}_{n_{11}} & \underline{1}_{n_{11}} & \underline{1}_{n_{11}} & \underline{0}_{n_{11}} & \cdots & \underline{0}_{n_{11}} \\
\underline{1}_{n_{21}} & \underline{1}_{n_{21}} & \underline{0}_{n_{21}} & \underline{1}_{n_{21}} & \cdots & \underline{0}_{n_{21}} \\
\vdots & \vdots & \vdots & \vdots & \ddots & \vdots \\
\underline{1}_{n_{I-1,1}} & \underline{1}_{n_{I-1,1}} & \underline{0}_{n_{I-1,1}} & \underline{0}_{n_{I-1,1}} & \cdots & \underline{1}_{n_{I-1,1}} \\
\underline{1}_{n_{11}} & \underline{1}_{n_{11}} & \underline{0}_{n_{11}} & \underline{0}_{n_{11}} & \cdots & \underline{0}_{n_{11}} \\
\underline{1}_{n_{12}} & -\underline{1}_{n_{12}} & \underline{1}_{n_{12}} & \underline{0}_{n_{12}} & \cdots & \underline{0}_{n_{12}} \\
\underline{1}_{n_{22}} & -\underline{1}_{n_{22}} & \underline{0}_{n_{22}} & \underline{1}_{n_{22}} & \cdots & \underline{0}_{n_{22}} \\
\vdots & \vdots & \vdots & \vdots & \ddots & \vdots \\
\underline{1}_{n_{I-1,2}} & -\underline{1}_{n_{I-1,2}} & \underline{0}_{n_{I-1,2}} & \underline{0}_{n_{I-1,2}} & \cdots & \underline{1}_{n_{I-1,2}} \\
\underline{1}_{n_{12}} & -\underline{1}_{n_{12}} & \underline{0}_{n_{12}} & \underline{0}_{n_{12}} & \cdots & \underline{0}_{n_{12}}
\end{matrix}
$$

The difference in column rank between the full and reduced RB models is (J-1) (I-1). This difference in column rank is due to the elimination of the interaction component ($\gamma_{ij} = 0$) in the reduced model; therefore, the difference between the reduced and full models' error sum of squares is due to interaction

$$SS_e^{(rb)} - SS_e^{(fb)} = SS_\gamma^{(fb)} \tag{7}$$

Functional relationships can be established between the sum of squares components for the RB and CANCOVA models by examining the least squares estimators for their respective parameter vectors

$$\underline{\hat{\theta}}^{(c)} = \left(\underline{X}^{(c)\prime}\underline{X}^{(c)}\right)^{-1}\underline{X}^{(c)\prime}\underline{Y}$$

$$\underline{\hat{\theta}}^{(rb)} = \left(\underline{X}^{(rb)\prime}\underline{X}^{(rb)}\right)^{-1}\underline{X}^{(rb)\prime}\underline{Y}$$

Since $\underline{Y}$ is the same score vector for both models and $\underline{X}^{(c)} = \underline{X}^{(rb)}$, the least squares estimators are equal:

$$\hat{\theta}^{(c)} = \begin{bmatrix} \mu^{(c)} \\ \alpha_1^{(c)} \\ b_1 \\ b_2 \\ \vdots \\ b_{I-1} \end{bmatrix} = \hat{\theta}^{(rb)} = \begin{bmatrix} \mu^{(rb)} \\ \alpha_1^{(rb)} \\ \beta_1 \\ \beta_2 \\ \vdots \\ \beta_{I-1} \end{bmatrix} = \begin{bmatrix} \bar{Y}... \\ \bar{Y}._1. \\ \bar{Y}_1.. \\ \bar{Y}_2.. \\ \vdots \\ \bar{Y}_{I-1,..} \end{bmatrix} \tag{8}$$

From equation 8 it can be shown that

$$SS_{\alpha}^{(rb)} = SS_{\alpha}^{(c)} = SS_{\alpha}^{(fb)} = SS_{\alpha}^{(hc)} \tag{9}$$

$$SS_{\beta}^{(rb)} = SS_{b}^{(c)} = SS_{\beta}^{(fb)} = SS_{b}^{(hc)} \tag{10}$$

By substituting the appropriate equalities from equations 9 and 10 into equations 6 and 7 it can be shown that

$$SS_h = SS_{\gamma}^{(fb)} \tag{11}$$

and since $X^{(c)} = X^{(rb)}$

$$SS_e^{(rb)} = SS_e^{(c)} \tag{12}$$

Table 3 lists all the component sums of squares for each model along with their respective degrees of freedom.

The difference in the power of the RB and CANCOVA analyses to detect treatment effects is dependent on both the amount of interaction (heterogeneity) present in the data and the difference in degrees of freedom associated with the mean square error term (MS_e) under each model. For the (fb) RB with I levels of blocking and J levels of treatment:

$$MS_e^{(fb)} = \frac{SS_e^{(fb)}}{n..-JI} \tag{13}$$

Table 3

SUM OF SQUARES AND DEGREES OF FREEDOM FOR RB AND CANCOVA ANALYSES

RB		CANCOVA	
Sum of Squares	Degrees of Freedom	Sum of Squares	Degrees of Freedom
SS_α	J-1	SS_α	J-1
SS_β	I-1	SS_b (regression)	I-1
SS_γ	(J-1) (I-1)	SS_e	n..-J-I+1
		---	---
SS_e	n..-JI	$SS_e^{(hc)}$	n..-JI
		SS_h	(J-1) (I-1)

Note. The SS_e for CANCOVA can be partitioned into $SS_e^{(hc)}$ which is the error sum of squares using separate within-cell regression coefficients, and SS_h which is the sum of squares for heterogenity of regression.

and for the (c) CANCOVA analysis with I-1 covariables and J levels of treatment

$$MS_e^{(c)} = \frac{SS_e^{(fb)}}{n..-J-I+1} \tag{14}$$

The F ratio for the treatment effect in the (fb) RB is

$$F_\alpha^{(fb)} = \frac{MS_\alpha}{MS_e^{(fb)}} = \frac{(n..-JI)MS_\alpha}{SS_e^{(fb)}} \tag{15}$$

and the F ratio for treatment effect in the (c) CANCOVA analysis is

$$F_\alpha^{(c)} = \frac{MS_\alpha}{MS_e^{(c)}} = \frac{(n..-J-I+1)MS_\alpha}{SS_e^{(c)}} \tag{16}$$

For sufficiently large sample sizes,

$$P\left(F_{\alpha}^{(fb)}\right) \tilde{=} P\left(F_{\alpha}^{(c)}\right)$$

where $P(F_{\alpha})$ is the probability that $F_{\alpha} \geq F_{\alpha}$ under the central F distribution when the sum of squares for interaction (heterogeneity) is equal to 0. When the sum of squares for interaction (heterogeneity) is not equal to 0, the following relationships hold:

$$P\left(F_{\alpha}^{(fb)}\right) < P\left(F_{\alpha}^{(c)}\right) \quad \text{when } F_h = F_{\gamma} > 1, \tag{17}$$

where F_h is the F ratio for testing the null hypothesis of homogeneity of regression and F_{γ} is the F ratio for testing the null hypothesis of no interaction.

$$P\left(F_{\alpha}^{(fb)}\right) > P\left(F_{\alpha}^{(c)}\right) \quad \text{when } F_h = F_{\alpha} < 1. \tag{18}$$

Given that $F_{\alpha}^{(fb)} > F_{\alpha}^{(c)}$ the proof of equations 17 and 18 is

$$\frac{(n..-JI)MS_{\alpha}}{SS_e^{(fb)}} > \frac{(n..-J-I+1)MS_{\alpha}}{SS_e^{(c)}}$$

Since $SS_e^{(c)} = SS_e^{(hc)} + SS_h$, then

$$\frac{(n..-JI)MS_{\alpha}}{SS_e^{(fb)}} > \frac{(n..-J-I+1)MS_{\alpha}}{SS_e^{(hc)} + SS_h} \tag{19}$$

Dividing by MS_{α}, expanding and combining like terms:

$$\frac{SS_h}{SS_e^{(hc)}} > \frac{(J-1)\ (I-1)}{(n..-JI)} \tag{20}$$

dividing both sides by (J-1) (I-1) / (n..-JI):

$$\frac{(n..-JI)SS_h}{(J-1)(I-1)SS_e^{(hc)}} > 1 \tag{21}$$

since

$$\frac{(n..-JI)SS_h}{(J-1)(I-1)SS_e^{(hc)}} = \frac{MS_h}{MS_e^{(hc)}} = F_h,$$

$$P\left(F_{\alpha}^{(fb)}\right) < P\left(F_{\alpha}^{(c)}\right) \quad \text{when } F_h = F_\gamma > 1.$$

From equations 17 and 18 it is clear that when $F_h = F_\gamma = 1$ the power of the RB (Pw(RB)) is equal to the power of the CANCOVA (Pw(C)), when $F_h = F_\gamma > 1$, Pw(RB) > Pw(C), and when $F_h = F_\gamma < 1$, Pw(C) > Pw(RB).

The above relationships only apply when the sample size is sufficiently large to negate any degrees of freedom differences in the distribution of the treatment effect F ratios for the RB and CANCOVA analyses. When the sample size is relatively small, the degrees of freedom difference between the two techniques, along with the level of heterogeneity (interaction) in the samples, plays an important part in determining the relative power of the two techniques.

The following arguments are used to define the functional relationship between the amount of heterogeneity (interaction) in small samples and the relative power of the RB and CANCOVA techniques. If the critical values at a given alpha level are known for $F_{\alpha}^{(fb)}$ and $F_{\alpha}^{(c)}$, and $SS_e^{(hc)} = SS_e^{(fb)}$ is defined as a constant equal to 1, then the value of the $SS_h = SS_\gamma^{(fb)}$ can be computed as a proportion of $SS_e^{(hc)} = SS_e^{(fb)}$; the value of this proportion when Pw(RB) = Pw(C) is defined as a pivot point value (PV) for the power function. When the ratio $SS_h/SS_e^{(hc)} = SS_\gamma^{(fb)} / SS_e^{(fb)} > PV$, Pw(RB) > Pw(C), but when this ratio is less than PV, then Pw(RB) < Pw(C).

Table 4 lists the PV values for alpha levels of .01 and .05 for two-, three-, and four-category control variables with sample sizes varying from (J·I·2) to (J·I·6) observations per cell. The algebraic argument used to compute the listed PV values is:

Given that $SS_e^{(fb)} = SS_e^{(hc)} = 1$, then

$$F_{\alpha}^{(fb)} = \frac{MS_{\alpha}}{n..-JI} \tag{22}$$

and

$$F_{\alpha}^{(c)} = \frac{MS_{\alpha}}{1 + SS_h/n..-J-I+1} \tag{23}$$

Table 4

PIVOT POINT VALUE FOR THE .01 AND .05 ALPHA LEVELS

Design Type	Total Sample Size	Alpha	
		.01 PV	.05 PV
Two-Category Variable Design	8	.5899	.4583
	12	.1994	.1690
	16	.1139	.1019
	20	.0791	.0728
	24	.0604	.0566
Three-Category Variable Design	12	.6277	.5013
	18	.2283	.2040
	24	.1371	.1270
	30	.0977	.0923
	36	.0770	.0729
Four-Category Variable Design	16	.6350	.5093
	24	.2377	.2183
	32	.1382	.1301
	40	.0997	.0982
	48	.0791	.0761

where $F_{\alpha}^{(fb)}$ and $F_{\alpha}^{(c)}$ are the critical values, at a given alpha level, of the treatment effect F ratios for the RB and CANCOVA designs respectively, and n.. is the total sample size. MS_{α} can be computed as a function of the known constants $F_{\alpha}^{(fb)}$, n..,J, and I using the relationship defined in Equation 22:

$$MS_{\alpha} = \frac{F_{\alpha}^{(fb)}}{n..-JI} = K. \qquad (24)$$

Substitution of K for MS_{α} in Equation 23 results in

$$F_{\alpha}^{(c)} = \frac{K}{1+SS_h/n..-J-I+1} \quad ;$$

transposing gives

$$SS_h = \frac{K(n..-J-I+1)}{F_{\alpha}^{(c)}} \qquad (25)$$

Since PV = $SS_h/SS_e^{(hc)}$ and $SS_e^{(fb)} - SS_e^{(hc)} = 1$, then PV = SS_h; therefore, Equation 25 can be used to compute PV values for different designs. Table 4 only lists PV values for designs with sample sizes up to a maximum of (J·I·6). Total sample sizes greater than (J·I·6) are sufficiently large that the differences in the distributions of the treatment effect F ratios for the RB and CANCOVA techniques are negligible and the PV values can be computed directly from Equation 20.

Empirical

None of the 12 null design combinations (eta = 0 and treatment effect = 0) produced significant lack of fit using a chi-square goodness-of-fit test. Table 5 contains summary data for the power comparisons between RB and CANCOVA for all 48 design combinations in which eta and the treatment effect were non-zero. The empirical power for each analysis is expressed as the proportion of samples in which the null hypothesis of no treatment effect was rejected. The empirical powers for both the RB and RCANCOVA analyses are given for the .01, .05, and .10 nominal alpha levels for each of the 48 design combinations. The power comparisons are divided into three groupings: (1) comparisons for the total number of samples generated (3,000), (2) comparisons for those samples in which interaction and heterogeneity were significant at the .01 level, and (3) comparisons for those samples in which interaction and heterogeneity were significant at the .05 level.

DISCUSSION

Analytical Results

If there is no interaction or heterogeneity of regression, the least-square estimators of parameters and error terms for RB and CANCOVA are identical. In situations where there is interaction or heterogeneity of regression, the error sum of squares for CANCOVA can be partitioned into a sum of squares for heterogeneity which equals the sum of squares for interaction under RB analysis, and an error sum of squares due to the use of separate within-cell regression coefficients which equals the error sum of squares under RB analysis. Given the situation where sample size is fixed and the number of observations per level of the control variable is a constant, RB and CANCOVA provide the same information.

Even though the two techniques can provide the same information in terms of component sums of squares, the relative powers of the two techniques are not necessarily equal. The power relationship was shown to be a function of sample size, design type, and amount of heterogeneity (interaction). For situations in which the heterogeneity (interaction) is equal to zero, $MS_e^{(c)} < MS_e^{(fb)}$; therefore, $P\left(F_\alpha^{(c)}\right) < P\left(F_\alpha^{(fb)}\right)$ making

Table 5

EMPIRICAL POWER LEVELS FOR THE 48 DESIGN COMBINATIONS IN WHICH ETA AND THE TREATMENT EFFECT WERE NON-ZERO

Marginal Ratio / Treatment Effect / Eta			Two-Category Designs, Sample Size 20: equal, .2, .3	equal, .2, .9	equal, .5, .3	equal, .5, .9	unequal, .2, .3	unequal, .2, .9	unequal, .5, .3	unequal, .5, .9
Model Power Comparisons										
	RB/RC									
	.01	RB	.0187	.0203	.0573	.0617	.0113	.0147	.0613	.0550
		RC	.0193	.0117	.0560	.0660	.0153	.0157	.0613	.0613
Nominal	.05	RB	.0707	.0683	.1813	.1317	.0520*	.0753	.1850	.1657
Alpha		RC	.0723	.0637	.1693	.1750	.0675	.0693	.1703	.1793
	.10	RB	.1233	.1280	.2900	.2810	.1100*	.1303	.2923*	.2787
		RC	.1357	.1213	.2790	.2773	.1280	.1313	.2690	.2860
	RB01/RC01									
	.01	RB	.0000	.0000	.1429	.1795	.0000	.0370	.1250	.0690
		RC	.0000	.0000	.0938	.0000	.0455	.0313	.0968	.0714
Nominal	.05	RB	.1111	.0508	.4286	.3077	.0345	.1111	.4688*	.2069
Alpha		RC	.0833	.0294	.1875	.1071	.1818	.0938	.1935	.2857
	.10	RB	.2222	.1765	.4286	.4103	.1379	.1481	.7188*	.2069
		RC	.1667	.0588	.3438	.2143	.2727	.1563	.4194	.2857
	RB05/RC05									
	.01	RB	.0128	.0301	.0725	.1143	.0147	.0325	.0621	.1269
		RC	.0064	.0126	.0361	.0857	.0164	.0145	.0405	.0970
Nominal	.05	RB	.1282	.0977	.1884	.2357	.1809	.0976	.2795**	.2687
Alpha		RC	.0641	.0440	.1325	.1657	.0656	.0870	.1216	.2388
	.10	RB	.1731	.1729	.2754	.3357	.1838	.1870	.4037**	.3060
		RC	.1474	.1132	.2590	.2514	.1311	.1522	.1959	.2761
			Sample Size 80							
	RB/RC									
	.01	RB	.0380*	.0473	.3207	.3413	.0397	.0503	.3307	.3507
		RC	.0507	.0480	.3407	.3390	.0420	.0443	.3453	.3410
Nominal	.05	RB	.1183**	.1443	.5603**	.6050	.1397	.1510	.5787	.5917
Alpha		RC	.1600	.1417	.6083	.5983	.1347	.1340	.5960	.5857
	.10	RB	.2103**	.2337	.6710**	.7120	.2180	.2347	.6813	.7110
		RC	.2510	.2210	.7220	.7080	.2247	.2213	.6920	.7073
	RB01/RC01									
	.01	RB	.0789	.0000	.2903	.5000	.0870	.0800	.7500**	.5769**
		RC	.1111	.0345	.4091	.4333	.0278	.1000	.2069	.1739
Nominal	.05	RB	.1579	.0714	.5806*	.6905	.1739	.2000	1.0000**	.3769
Alpha		RC	.2222	.1034	.7727	.6667	.1944	.4000	.5172	.4348
	.10	RB	.2368	.1429	.6774	.6905	.2174	.3200	1.0000**	.7692
		RC	.2222	.2414	.8182	.7667	.2778	.4500	.7241	.6087
	RB05/RC05									
	.01	RB	.0426	.0274	.2933	.3893	.0507	.0816	.3462	.3643
		RC	.0643	.0432	.3154	.3431	.0405	.0496	.4118	.3660
Nominal	.05	RB	.1560	.1438	.5333	.6242	.1304	.2109	.5897	.6214
Alpha		RC	.2071	.1481	.6174	.5693	.1486	.1631	.5948	.5752
	.10	RB	.2057	.2808	.6200	.6913	.1957	.2925	.5897*	.7214
		RC	.2714	.2407	.7114	.6788	.2027	.2199	.7190	.6667

Table 5 - Continued

Three-Category Designs

Sample Size 36

Marginal Ratio / Treatment Effect / Eta			equal .2 .3	equal .2 .9	equal .5 .3	equal .5 .9	unequal .2 .3	unequal .2 .9	unequal .5 .3	unequal .5 .9
Model Power Comparisons										
	RB/RC									
	.01	RB	.0163	.0177	.1443	.1210	.0207	.0240	.1197	.1480
		RC	.0200	.0147	.1307	.1180	.0170	.0190	.1203	.1310
Nominal	.05	RB	.0763	.0747	.3453	.2923	.0827	.0823	.3077	.3110
Alpha		RC	.0863	.0763	.3277	.2980	.0790	.0887	.2953	.3035
	.10	RB	.1520	.1527	.4843**	.4183	.1523	.1510	.4223	.4377
		RC	.1537	.1407	.4500	.4117	.1493	.1487	.4153	.4233
	RB01/RC01									
	.01	RB	.0667	.1176	.3000	0833	.1154	.1579*	.0690	.5000**
		RC	.0000	.0000	.1500	.0370	.0345	.0263	.1333	.1143
Nominal	.05	RB	.2000*	.1765	.5500	.2083	.1923	.1579	.3448	.5000**
Alpha		RC	.0645	.0952	.3500	.1852	.1724	.1316	.3333	.1714
	.10	RB	.3000	.4118	.6500	.2500	.2692	.1579	.3793	.5000
		RC	.0968	.1429	.4000	.2593	.2759	.1842	.4333	.3429
	RB05/RC05									
	.01	RB	.0301	.0567	.2444*	.1293	.0556	.0460	.2418*	.2690*
		RC	.0136	.0204	.1437	.1020	.0252	.0291	.1497	.1523
Nominal	.05	RB	.1566*	.1277	.4444	.3265	.1528	.1034	.6144**	.3655
Alpha		RC	.0816	.0884	.3438	.2517	.0881	.1047	.3293	.3113
	.10	RB	.2349	.2482*	.5704	.3946	.2431	.1667	.7320**	.5172
		RC	.1565	.1361	.4500	.3197	.1572	.2035	.4551	.4437

Sample Size 144

	RB/RC									
	.01	RB	.0847	.0860	.6400	.6373	.0847	.0793	.6550	.6277
		RC	.0850	.0817	.6600	.6427	.0873	.0853	.6460	.6357
Nominal	.05	RB	.2240	2210	.8433	.8347	.2230	.2120	.8563	.8430
Alpha		RC	.2247	.2237	.8400	.8313	.2250	.2180	.8577	.8423
	.10	RB	.3227	.3173	.9040	.9857	.3243	.3193	.9140	.9123
		RC	.3323	.3267	.9077	.9080	.3337	.3273	.9177	.9073
	RB01/RC01									
	.01	RB	.0000	.1667	.5455	.6800	.1379	.0541	.5152	.5000
		RC	.0000	.0870	.6522	.7333	.1818	.0750	.3415	.6585
Nominal	.05	RB	.0556*	.4333	.9091	.7200	.2414	.1892	.7576	.7368
Alpha		RC	.3600	.2174	.7609	.8667	.3182	.2500	.7073	.8573
	.10	RB	.1111	.5000	.9545	.8400	.2759	.2973	.9697	.9737
		RC	.3600	.3043	.8478	.9333	.3636	.3500	.8537	.9756
	RB05/RC05									
	.01	RB	.0682	.0701	.6395	.7105	.1500*	.0588	.6017	.6493
		RC	.0511	.0612	.6187	.6954	.0687	.0963	.5633	.6434
Nominal	.05	RB	.1894	.2739	.8571	.8750	.7929	.1830	.8559	.8881
Alpha		RC	.2117	.2585	.7937	.8675	.2977	.2444	.8608	.8601
	.10	RB	.2197*	.3694	.9252	.9211	.3786	.2876	.9237	.9776
		RC	.3431	.3333	.8812	.9470	.3740	.3556	.9114	.9441

Table 5 - Continued

Four-Category Designs

Marginal Ratio			Sample Size 56 equal				Sample Size 56 unequal			
Treatment Effect			.2		.5		.2		.5	
Eta			.3	.9	.3	.9	.3	.9	.3	.9
Model Power Comparisons										
	RB/RC									
	.01	RB	.0317	.0290	.2353*	.2193	.0320	.0300	.2173	.2247
		RC	.0297	.0303	.2097	.2107	.0323	.0280	.2220	.2240
Nominal	.05	RB	.1113	.1143	.4757*	.4460	.1130	.1183*	.4547	.4667
Alpha		RC	.1070	.1130	.4380	.4250	.1053	.1020	.4540	.4503
	.10	RB	.1863	.1983	.6020*	.5647	.1750	.1917	.5743	.5900
		RC	.1820	.1893	.5720	.5503	.1780	.1817	.5703	.5733
	RB01/RC01									
	.01	RB	.0500	.0667	.3200	.3636*	.2222*	.1290	.4231**	.3793
		RC	.0000	.0000	.3000	.1250	.0000	.0303	.1143	.2857
Nominal	.05	RB	.2000	.1333	.5200	.5455	.4074*	.1290	.5769	.5862
Alpha		RC	.0625	.1000	.5000	.3000	.0870	.1212	.4000	.4286
	.10	RB	.2500	.2000	.6800	.7727**	.5185*	.1613	.6154	.7586*
		RC	.0938	.2000	.6000	.4250	.2174	.2424	.5714	.5000
	RB05/RC05									
	.01	RB	.0268	.0526	.3116	.2727**	.0671	.0657	.2463	.2500
		RC	.0192	.0129	.2553	.1355	.0355	.0311	.1667	.2171
Nominal	.05	RB	.1342	.1382	.5072	.5035	.2256	.1168	.5075	.5405*
Alpha		RC	.1026	.0839	.4610	.3871	.1560	.0745	.4058	.3953
	.10	RB	.2081	.2039	.6522	.6364*	.2988	.1606	.5896	.6484*
		RC	.1923	.1871	.6099	.1903	.2482	.1739	.5362	.5116
			Sample Size 224							
	RB/RC									
	.01	RB	.1337	.1467	.8743	.8727	.1237	.1387	.8730	.8693
		RC	.1363	.1420	.8740	.8717	.1230	.1387	.8633	.8683
Nominal	.05	RB	.3180	.3340	.9637	.9660	.3230	.3173	.9640	.9633
Alpha		RC	.3323	.3267	.9583	.9630	.3180	.3313	.9593	.9640
	.10	RB	.4413	.4617	.9843	.9840	.4573	.4417	.9823	.9837
		RC	.4550	.4497	.9790	.9810	.4453	.4453	.9833	.9813
	RB01/RC01									
	.01	RB	.1765	.0690	.8571	.9355	.2083	.1379	.7917	.7241
		RC	.0417	.1034	.8333	.8788	.1538	.1563	.7727	.8571
Nominal	.05	RB	.2941	.2069	1.0000	1.0000	.3333	.2414	.9583	.9310
Alpha		RC	.2083	.1379	1.0000	.9697	.3462	.2500	.9545	.9286
	.10	RB	.3235	.3448	1.0000	1.0000	.4583	.4138	1.0000	1.0000
		RC	.3333	.3448	1.0000	.9697	.4615	.3750	.9545	1.0000
	RB05/RC05									
	.01	RB	.1400	.679	.8599	.9167	.1799	.1419	.8767	.8553
		RC	.0800	.1678	.9091	.9006	.1214	.1594	.8414	.8130
Nominal	.05	RB	.2400	.3212	.9682	.9848	.3237	.3378	.9521	.9623
Alpha		RC	.2560	.3221	.9805	.9752	.3500	.3623	.9517	.9512
	.10	RB	.3333	.4380	.9809	.9924	.4676	.4257	.9795	.9874
		RC	.3680	.4698	.9935	.9876	.4286	.4928	.9862	.9837

* Power comparison significantly different at .05 level.
** Power comparison significantly different at .01 level.

Note: RC = RCANCOVA.
RB01 = RB analysis in which interaction is significant at the .01 level.
RB05 = RB analysis in which interaction is significant at the .05 level.
RC01 = RCANCOVA analysis in which heterogeneity is significant at the .01 level.
RC05 = RCANCOVA analysis in which heterogeneity is significant at the .05 level.
/ = Compared with.

the CANCOVA technique uniformly more powerful under these conditions. This relationship does not hold, however, for conditions in which $SS_h = SS_\gamma$ is not equal to 0. A value equal to the ratio, $SS_h / SS_e^{(hc)} =$ PV, was defined for situations in which Pw(RB) = Pw(C). PV was shown to be a monotonic decreasing function of sample size. The smaller sample sizes, within any design type, allow for the greatest amounts of heterogeneity (interaction) before Pw(RB) > Pw(C); in addition, the degrees of freedom difference between RB and CANCOVA techniques [df(c) > df(b)] provides a higher probability for rejection of the null hypothesis of no treatment effect under CANCOVA. These combined factors provide CANCOVA with a power advantage when the sample size is relatively small and heterogeneity (interaction) is minimal. As the sample size increases, PV approaches 0 and at the same time the degrees of freedom advantage of CANCOVA over RB becomes negligible. Under these conditions the power advantage tends to shift in favor of RB. The relatively small PV of the larger sample size does not permit very high levels of heterogeneity (interaction) to be present before Pw(RB) becomes greater than Pw(C).

Another aspect of the analytical results involves the implications regarding the relationship between power and the correlation between the concomitant variable and the criterion variable. For RB and ANCOVA designs in which the concomitant variables are continuous, the relative power of each design is dependent on the value of the linear correlation coefficient (ρ) between the concomitant and criterion variables. When $\rho < 0.4$, the RB technique tends to be more powerful; when $\rho > 0.6$, the ANCOVA technique tends to be more powerful.[17] However, these results do not generalize to the case when the concomitant variable is not continuous. In RB, the qualitative concomitant variable is used as a blocking variable and the amount of error reduction is equal to the sum of squares for blocking, SS_β; in CANCOVA, the qualitative concomitant variable is used to generate a set of dichotomous dummy covariables and the amount of error reduction is equal to the sum of squares for the pooled within-cell regression, SS_b. It was shown that $SS_\beta = SS_b$; since the amount of error variability explained by the correlation between the qualitative concomitant and criterion variables for both the RB and CANCOVA analyses is the same, the power difference between the RB or CANCOVA techniques is not a function of the value of eta.

Empirical Results

Goodness-of-fit. Using chi-square goodness-of-fit tests, it was shown that all 12 null design combinations produced chi-square statistics which fell within 95% confidence intervals for chance occurrences. These sample runs were used to establish the adequacy of the random number generators and to empirically validate the computational subroutines within the Monte Carlo program.

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[17] Cox, 1957, op. cit.; Feldt, 1958, op. cit.

Empirical Power Comparisons. The empirical data showed no interpretable differences, either in magnitude and direction, between the power of the RB and RCANCOVA. At nominal alpha levels of .01, .05, and .10 the number of significantly different power comparisons for the two-, three-, and four-category design types fell within 95% confidence intervals for chance occurrences. Empirically there is no difference in power between the two techniques; however, caution must be exercised in interpreting these results to an applied situation. RCANCOVA was designed as a post-hoc CANCOVA in which the a priori fixed number of observations per category of the concomitant variable ($n_{i}.$) becomes a post hoc random variable. Since CANCOVA and RB were shown to be analytically equivalent, RCANCOVA could be viewed as a post hoc blocking technique; however, in actual practice post hoc blocking is a technique in which the a priori fixed number of observations per cell of the design (n_{ij}) becomes a post hoc random variable. Since $n_{i}. \neq n_{ij}$, the RCANCOVA is not equivalent to a post hoc blocking technique; further investigation using more realistic post hoc models is necessary before inferences can be drawn regarding the relative power of RB and post hoc RB designs.

SUMMARY

The powers of fixed-effects randomized block (RB) and analysis of covariance (CANCOVA) using qualitative concomitant variables were analytically and empirically compared. Analytical comparisons were made of the powers of RB and CANCOVA in which the number of observations (n_{i}) within each of the I categories of the concomitant variable was a constant. Empirical comparisons were made of the power of CANCOVA in which n_{i} was a random variable (RCANCOVA) with RB in which n_{i} was a constant. A Monte Carlo program simulated fixed-effects analyses with two levels of treatment, one criterion variable, and a qualitative concomitant variable with I categories. Three design types in which I was equal to 2, 3, and 4 were studied. The parameters varied for each design type were: (1) total sample size (n..) (I=2, n..=20, 80; I=3, n..=36, 144; I=4, n..=56, 224), (2) ratio of number of row observations (I=2, 1:1, 4:1; I=3, 1:1:1, 4:1:1; I=4, 1:1:1:1, 4:1:1:1), (3) eta (0.0, 0.3, 0.9), and (4) magnitude of treatment effect (0.0, 0.2, 0.5).

Analytically, the RB and CANCOVA provided the same information in terms of component sums of squares. However, the power relationship was shown to be a function of sample size, design type, and amount of heterogeneity (interaction) present. Empirically no interpretable differences were found, either in magnitude and direction, between the power of the RB and RCANCOVA for any of the design type and parameter combinations studied.

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APPENDIXES

APPENDIX A

DOCUMENTATION FOR PROGRAM WHICH COMPUTES ETA FROM MARGINAL ROW MEANS

A single parameter card is used for each combination of marginal means tested.

Col	Information
1	number of categories in the control variable (2, 3, or 4)
2-4	value of marginal mean for first blocking level (F3.2)
5-7	value of marginal mean for second blocking level (F3.2)
8-10	value of marginal mean for third blocking level (F3.2)
11-13	value of marginal mean for fourth blocking level (F3.2)

(Card contains only as many means as there are categories, i.e., a three category control variable design has only three marginal mean values punched.)

The program reads in the marginal mean values and computes the value of eta based on the following relationship:

$$\eta^2 = \frac{1}{I\sigma_T^2} \sum_{i=1}^{I} (\mu_{i.} - \mu_{..})^2$$

where I = number of rows, $\mu_{i.}$ = row mean, $\mu_{..}$ = grand mean, σ_T^2 is the total variance and is set equal to one.

Output consists of the values of the row means and the computed etas.

Table A-1 shows the row means for each eta used in the Monte-Carlo analyses.

Table A-1

ROW MEAN VALUES FOR EACH DESIGN TYPE

	Control Variable Designs								
Value of Eta	Two Category		Three Category			Four Category			
	Row		Row			Row			
	1	2	1	2	3	1	2	3	4
.3	1.00	1.60	1.00	1.30	1.70	1.00	1.20	1.40	1.80
.9	1.00	2.80	1.00	1.60	3.75	1.00	1.60	2.20	3.40

The row means in Table A-1 were used to compute the actual cell means for each cell in a specific design. The computed cell means were used by RANDN to generate the observations within that cell. For example, if the value of eta was .3 and the treatment effect was .2 for a two category design, then cell (11)'s population mean would be 0.90, cell (12)'s population mean would be 1.10, cell (21)'s population mean would be 1.50, and cell (22)'s population mean would be 1.70.

APPENDIX B

DOCUMENTATION FOR THE CHI-SQUARE GOODNESS-OF-FIT TEST

A single parameter card was used for each starting number to be tested.

Col	Information
1-6	six digit starting number read in under F format (F6.0)

The program read in the starting number and called up the subroutine RANDN to generate 1,000 numbers, with a mean of 0.0 and standard deviation of one. The program divides the empirical frequency distribution into 16 intervals of .4 standard deviations each. Observed frequencies are compared with the expected for each interval and chi squares for each interval as well as an overall chi square are computed.

Output consists of the overall chi square, observed frequencies for each of the 16 intervals and chi squares for each interval.

APPENDIX C

DOCUMENTATION FOR MONTE CARLO PROGRAM

The following parameter cards are used by the Monte Carlo program:

(1) Starting Number Card

Col	Information
1-6	six digit starting number for RANDN (F6.0)
7-12	six digit starting number for RANDU (F6.0)
13-16	four digit field for number of sample generations to be run for each design combination read in on the following parameter cards (I4)

(2)

Col	
1	0 stop program 1 read in parameters and run
2	one digit F field for the value of eta, (F1.1)
3-5	three digit F field for treatment effect difference, (F3.2)
6	one digit I field for number of cells in the RB analysis 4, 6, or 8
7-8	two digit I field defining number of observations to be generated by RANDN in cell (11)
9-11	three digit F field defining the value of the population mean under which the observations in cell (11) will be generated by RANDN, (F3.2)
12-13	two digit I field defining number of observations to be generated by RANDN in cell (21)
14-16	three digit F field defining the value of the population mean under which the observations in cell (21) will be generated by RANDN, (F3.2)

Col	
.	...
.	...
.	...

repeat until parameters for all cells involved in the particular design are punched. The maximum allowed is for a 4 x 2 design with 8 cells.

The program reads in the parameter cards; card (1) is read only once for each set of runs. There is no limit to the number of type (2) cards which may be read using a single set of starting numbers. If it is desirable to use different sets of starting numbers for each design combination, then the program must be terminated after each run and a new set of parameter cards, both (1) and (2), used to start a new run.

RANDN is called up to generate data for each cell of the design. The data generated are analyzed by the RB subroutine. A new set of data is generated by RANDN under subroutine RCANCOVA. This set has the same number of observations as the RB analysis, but the number of observations per control variable category is determined by a subroutine which randomly assigns category classifications to each observation generated by RANDN. The probability of assigning a set of covariable scores corresponding to a given category of the concomitant variable is the ratio of the number of observations in that category to the total number of observations for the design. RCANCOVA produces a set of data in which the number of observations in each category of the control variable is a random variable and is representative of the population defined in the parameter card. Each observation within a category is assigned the proper set of covariable scores and the set of data generated under RCANCOVA is analyzed as if it were a one-way ANCOVA.

Output consists of a table containing the following:

1. The design parameters
    a. The dimensions of the design
    b. Eta
    c. Treatment effect difference
    d. Cell sizes, treatment level sizes, blocking level sizes, and the total sample size
2. Total sample data--summary data for all 6,000 samples generated
    a. Frequency of rejection, proportion of rejection, z and z' statistics at all six nominal alpha levels for the RB and RCANCOVA analyses
    b. A total frequency count for each analytical technique
3. Interaction and heterogeneity at the .01 level--data for all samples out of the 6,000 with interaction in the RB analysis significant at the .01 level and heterogeneity in the RCANCOVA analyses significant at the .01 level
    a. Same as 2a above
    b. Same as 2b above

4. Same as 3 above except for interaction and heterogeneity which was significant at the .05 level.

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1 USA Concept Anal Agcy, Bethesda, ATTN: MOCA-JF
1 USA Artic Test Ctr, APO Seattle, ATTN: STEAC-MO-ASL
1 USA Artic Test Ctr, APO Seattle, ATTN: AMSTE-PL-TS
1 USA Armament Cmd, Redstone Arsenal, ATTN: ATSK-TEM
1 USA Armament Cmd, Rock Island, ATTN: AMSAR-TDC
1 FAA-NAFEC, Atlantic City, ATTN: Library
1 FAA-NAFEC, Atlantic City, ATTN: Hum Engr Br
1 FAA Aeronautical Ctr, Oklahoma City, ATTN: AAC-44D
2 USA Fld Arty Sch, Ft Sill, ATTN: Library
1 USA Armor Sch, Ft Knox, ATTN: Library
1 USA Armor Sch, Ft Knox, ATTN: ATSB-DI-E
1 USA Armor Sch, Ft Knox, ATTN: ATSB-DT-TP
1 USA Armor Sch, Ft Knox, ATTN: ATSB-CD-AD
2 HQUSACDEC, Ft Ord, ATTN: Library
1 HQUSACDEC, Ft Ord, ATTN: ATEC-EX-E-Hum Factors
2 USAEEC, Ft Benjamin Harrison, ATTN: Library
1 USAPACDC, Ft Benjamin Harrison, ATTN: ATCP-HR
1 USA Comm-Elect Sch, Ft Monmouth, ATTN: ATSN-EA
1 USAEC, Ft Monmouth, ATTN: AMSEL-CT-HDP
1 USAEC, Ft Monmouth, ATTN: AMSEL-PA-P
1 USAEC, Ft Monmouth, ATTN: AMSEL-SI-CB
1 USAEC, Ft Monmouth, ATTN: C, Facl Dev Br
1 USA Materials Sys Anal Agcy, Aberdeen, ATTN: AMXSY-P
1 Edgewood Arsenal, Aberdeen, ATTN: SAREA-BL-H
1 USA Ord Ctr & Sch, Aberdeen, ATTN: ATSL-TEM-C
2 USA Hum Engr Lab, Aberdeen, ATTN: Library/Dir
1 USA Combat Arms Tng Bd, Ft Benning, ATTN: Ad Supervisor
1 USA Infantry Hum Rsch Unit, Ft Benning, ATTN: Chief
1 USA Infantry Bd, Ft Benning, ATTN: STEBC-TE-T
1 USASMA, Ft Bliss, ATTN: ATSS-LRC
1 USA Air Def Sch, Ft Bliss, ATTN: ATSA CTD-ME
1 USA Air Def Sch, Ft Bliss, ATTN: Tech Lib
1 USA Air Def Bd, Ft Bliss, ATTN: FILES
1 USA Air Def Bd, Ft Bliss, ATTN: STEBD-PO
1 USA Cmd & General Stf College, Ft Leavenworth, ATTN: Lib
1 USA Cmd & General Stf College, Ft Leavenworth, ATTN: ATSW-SE-L
1 USA Cmd & General Stf College, Ft Leavenworth, ATTN: Ed Advisor
1 USA Combined Arms Cmbt Dev Act, Ft Leavenworth, ATTN: DepCdr
1 USA Combined Arms Cmbt Dev Act, Ft Leavenworth, ATTN: CCS
1 USA Combined Arms Cmbt Dev Act, Ft Leavenworth, ATTN: ATCASA
1 USA Combined Arms Cmbt Dev Act, Ft Leavenworth, ATTN: ATCACO-E
1 USA Combined Arms Cmbt Dev Act, Ft Leavenworth, ATTN: ATCACC-CI
1 USAECOM, Night Vision Lab, Ft Belvoir, ATTN: AMSEL-NV-SD
3 USA Computer Sys Cmd, Ft Belvoir, ATTN: Tech Library
1 USAMERDC, Ft Belvoir, ATTN: STSFB-DQ
1 USA Eng Sch, Ft Belvoir, ATTN: Library
1 USA Topographic Lab, Ft Belvoir, ATTN: ETL-TD-S
1 USA Topographic Lab, Ft Belvoir, ATTN: STINFO Center
1 USA Topographic Lab, Ft Belvoir, ATTN: ETL-GSL
1 USA Intelligence Ctr & Sch, Ft Huachuca, ATTN: CTD-MS
1 USA Intelligence Ctr & Sch, Ft Huachuca, ATTN: ATS-CTD-MS
1 USA Intelligence Ctr & Sch, Ft Huachuca, ATTN: ATSI-TE
1 USA Intelligence Ctr & Sch, Ft Huachuca, ATTN: ATSI-TEX-GS
1 USA Intelligence Ctr & Sch, Ft Huachuca, ATTN: ATSI-CTS-OR
1 USA Intelligence Ctr & Sch, Ft Huachuca, ATTN: ATSI-CTD-DT
1 USA Intelligence Ctr & Sch, Ft Huachuca, ATTN: ATSI-CTD-CS
1 USA Intelligence Ctr & Sch, Ft Huachuca, ATTN: DAS/SRD
1 USA Intelligence Ctr & Sch, Ft Huachuca, ATTN: ATSI-TEM
1 USA Intelligence Ctr & Sch, Ft Huachuca, ATTN: Library
1 CDR, HQ Ft Huachuca, ATTN: Tech Ref Div
2 CDR, USA Electronic Prvg Grd, ATTN: STEEP-MT-S
1 CDR, Project MASSTER, ATTN: Tech Info Center
1 Hq MASSTER, USATRADOC, LNO
1 Research Institute, HQ MASSTER, Ft Hood
1 USA Recruiting Cmd, Ft Sheridan, ATTN: USARCPM-P
1 Senior Army Adv., USAFAGOD/TAC, Elgin AF Aux Fld No. 9
1 HQ USARPAC, DCSPER, APO SF 96558, ATTN: GPPE-SE
1 Stimson Lib, Academy of Health Sciences, Ft Sam Houston
1 Marine Corps Inst., ATTN: Dean-MCI
1 HQUSMC, Commandant, ATTN: Code MTMT 51
1 HQUSMC, Commandant, ATTN: Code MPI-20
2 USCG Academy, New London, ATTN: Admission
2 USCG Academy, New London, ATTN: Library
1 USCG Training Ctr, NY, ATTN: CO
1 USCG Training Ctr, NY, ATTN: Educ Svc Ofc
1 USCG, Psychol Res Br, DC, ATTN: GP 1/62
1 HQ Mid-Range Br, MC Det, Quantico, ATTN: P&S Div

1 US Marine Corps Liaison Ofc, AMC, Alexandria, ATTN: AMCGS-F
1 USATRADOC, Ft Monroe, ATTN: ATRO-ED
6 USATRADOC, Ft Monroe, ATTN: ATPR-AD
1 USATRADOC, Ft Monroe, ATTN: ATTS-EA
1 USA Forces Cmd, Ft McPherson, ATTN: Library
2 USA Aviation Test Bd, Ft Rucker, ATTN: STEBG-PO
1 USA Agcy for Aviation Safety, Ft Rucker, ATTN: Library
1 USA Agcy for Aviation Safety, Ft Rucker, ATTN: Educ Advisor
1 USA Aviation Sch, Ft Rucker, ATTN: PO Drawer O
1 HQUSA Aviation Sys Cmd, St Louis, ATTN: AMSAV-ZDR
2 USA Aviation Sys Test Act., Edwards AFB, ATTN: SAVTE-T
1 USA Air Def Sch, Ft Bliss, ATTN: ATSA TEM
1 USA Air Mobility Rsch & Dev Lab, Moffett Fld, ATTN: SAVDL-AS
1 USA Aviation Sch, Res Trng Mgt, Ft Rucker, ATTN: ATST-T-RTM
1 USA Aviation Sch, CO, Ft Rucker, ATTN: ATST-D-A
1 HQ, USAMC, Alexandria, ATTN: AMXCD-TL
1 HQ, USAMC, Alexandria, ATTN: CDR
1 US Military Academy, West Point, ATTN: Serials Unit
1 US Military Academy, West Point, ATTN: Ofc of Milt Ldrshp
1 US Military Academy, West Point, ATTN: MAOR
1 USA Standardization Gp, UK, FPO NY, ATTN: MASE-GC
1 Ofc of Naval Rsch, Arlington, ATTN: Code 452
3 Ofc of Naval Rsch, Arlington, ATTN: Code 458
1 Ofc of Naval Rsch, Arlington, ATTN: Code 450
1 Ofc of Naval Rsch, Arlington, ATTN: Code 441
1 Naval Aerospc Med Res Lab, Pensacola, ATTN: Acous Sch Div
1 Naval Aerospc Med Res Lab, Pensacola, ATTN: Code L51
1 Naval Aerospc Med Res Lab, Pensacola, ATTN: Code L5
1 Chief of NavPers, ATTN: Pers-OR
1 NAVAIRSTA, Norfolk, ATTN: Safety Ctr
1 Nav Oceanographic, DC, ATTN: Code 6251, Charts & Tech
1 Center of Naval Anal, ATTN: Doc Ctr
1 NavAirSysCom, ATTN: AIR-5313C
1 Nav BuMed, ATTN: 713
1 NavHelicopterSubSqua 2, FPO SF 96601
1 AFHRL (FT) William AFB
1 AFHRL (TT) Lowry AFB
1 AFHRL (AS) WPAFB, OH
2 AFHRL (DOJZ) Brooks AFB
1 AFHRL (DOJN) Lackland AFB
1 HQUSAF (INYSD)
1 HQUSAF (DPXXA)
1 AFVTG (RD) Randolph AFB
3 AMRL (HE) WPAFB, OH
2 AF Inst of Tech, WPAFB, OH, ATTN: ENE/SL
1 ATC (XPTD) Randolph AFB
1 USAF AeroMed Lib, Brooks AFB (SUL-4), ATTN: DOC SEC
1 AFOSR (NL), Arlington
1 AF Log Cmd, McClellan AFB, ATTN: ALC/DPCRB
1 Air Force Academy, CO, ATTN: Dept of Bel Scn
5 NavPers & Dev Ctr, San Diego
2 Navy Med Neuropsychiatric Rsch Unit, San Diego
1 Nav Electronic Lab, San Diego, ATTN: Res Lab
1 Nav TrngCen, San Diego, ATTN: Code 9000-Lib
1 NavPostGraSch, Monterey, ATTN: Code 55Aa
1 NavPostGraSch, Monterey, ATTN: Code 2124
1 NavTrngEquipCtr, Orlando, ATTN: Tech Lib
1 US Dept of Labor, DC, ATTN: Manpower Admin
1 US Dept of Justice, DC, ATTN: Drug Enforce Admin
1 Nat Bur of Standards, DC, ATTN: Computer Info Section
1 Nat Clearing House for MH-Info, Rockville
1 Denver Federal Ctr, Lakewood, ATTN: BLM
12 Defense Documentation Center
4 Dir Psych, Army Hq, Russell Ofcs, Canberra
1 Scientific Advsr, Mil Bd, Army Hq, Russell Ofcs, Canberra
1 Mil and Air Attache, Austrian Embassy
1 Centre de Recherche Des Facteurs, Humaine de la Defense Nationale, Brussels
2 Canadian Joint Staff Washington
1 C/Air Staff, Royal Canadian AF, ATTN: Pers Std Anal Br
3 Chief, Canadian Def Rsch Staff, ATTN: C/CRDS(W)
4 British Def Staff, British Embassy, Washington
1 Def & Civil Inst of Enviro Medicine, Canada
1 AiR CRESS, Kensington, ATTN: Info Sys Br
1 Militaerpsykologisk Tjeneste, Copehagen
1 Military Attache, French Embassy, ATTN: Doc Sec
1 Medecin Chef, C.E.R.P.A.-Arsenal, Toulon/Naval France
1 Prin Scientific Off, Appl Hum Engr Rsch Div, Ministry of Defense, New Delhi
1 Pers Rsch Ofc Library, AKA, Israel Defense Forces
1 Ministeris van Defensie, DOOP/KL Afd Sociaal Psychologische Zaken, The Hague, Netherlands